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the commemoration of the dead; but the fact that the "cove," or holy of holies, in the centre of the northern circle, faced the sun when rising at midsummer has been regarded as indicating sun-worship to have been the chief purpose of this vast monument, which was in all respects so suitable for a place of assembly for a tribe or nation.

A short distance to the north of the main road from Marlborough to Abury are the remains of a dolmen called the "Devil's Den," and there is another at Rockwell, four miles northwest from Marlborough and two miles northeast from Abury. There was also a circle at Winterbourne Bassett, four miles north from Abury, but it is not worth the trouble of a visit, as only three or four stones remain.

PHYSIOLOGICAL CONTRIBUTIONS FROM MISSOURI BOTANICAL GARDEN. I.

BY J. CHRISTIAN BAY, MISSOURI BOTANICAL GARDEN, ST. LOUIS, MO.

THE PLANT CELL.

IN the early part of this year, Professor von Sachs, of Wuerzburg, published a paper on the theory of cells: *Beitraege zur Zellentheorie* in "Flora," 1892, Heft 1, pp. 57-64. The leading thought of this publication seems to me to form, when combined with the following suggestions, the key and basis for deductions from the very long and interesting series of facts which forms the results of investigations of the later years in the functions of vegetable cells, both mechanical and physiological.

It is not difficult to trace how, even since the epoch of natural philosophy ("die Naturphilosophie"), the science of vegetable physiology has been in want of a solid foundation, a base, upon which the results of investigations in the phenomena of the life of vegetable cells could be firmly built. In the *Botanische Zeitung* a lance was in vain broken for the old theory; somebody then in vain put out the question, what Schleiden would give us instead of the old natural philosophy. Schleiden made no answer, because he had none to give.

The physiology of the plant cell having had since that time no leading exponent is, I suppose, the reason why at present that science merely consists of a series of very interesting, suggestive facts, but without the necessary conjunction with regard to points of view leading to general results.

A great many prominent men have devoted their lives to the study of vegetable cells, and we must allow that botany has now progressed as far as zoölogy, but only with regard to the accumulation of facts, in animal biology the cellular physiology of Virchow, dating from 1858, has arrived at a very high stage of development. Therefore, when thinking of the construction of a comparative physiology of animals and plants, it will be a most thankworthy task to collect all of the thrown facts concerning the physiology (qua biology) of the plant cell and arrange them from a general point of view.

The reason why the botanical part of cellular science has not brought forth general results during this long period is also to be sought in the definition of the cell body in botany. Very few physiologists would allow that the plant cell as well as the animal cell is an organism. Still this definition is to be looked at as a necessary foundation for a clear perception of the phenomena of botanical cellular physiology, both mechanical and chemical. As far back as 1848, one of the most prominent physiologists, N. Pringsheim (*De forma et incremento stratorum crassiorum in plantarum cellula observationes quaedam novae*. Halae, 1848, p. 38.) reminded us that "cellula est individuum," Hilger and Husemann, Weiss, and A. Zimmermann have told us almost the same, but still we find such definitions as "Grundorgan" (Frank), "Elementargebilde," "Formelemente" (G. Haberlandt). In his excellent "Lectures," Vines calls the plant cell "the physical basis of life." It must be remembered that Huxley ("Physical Basis of Life") only spoke of the protoplasm as the bearer of life. And Huxley himself, when he gave this most ingenious definition, did not see in protoplasm the *physiological basis of life*. Life never rested on a physical basis, nor consisted in physical matters alone.

Nobody will doubt whether a yeast cell is an organism or not.

Professor R. Pedersen, of Copenhagen, for six years my teacher in physiology, first mentioned these facts to me in the winter of 1891, acknowledging the results of this consideration for the evolution of cell theory in botany. Never this explanation was said with regard to the fact that said definition subsequently would form the key to cellular physiology in botany and, I may add, to comparative physiology of animals and plants.

The question is of considerable importance, because the accumulated facts now need a basis. The proposition of Sachs in his recent paper must be said to have come in due time. Yet it evidently ought to be connected with the given definition of the cell. Now we shall be able to arrange the facts in a system, see where vacant spaces may be, and fill up the voids, but up to the present time we were unable to do so.

Taking the "energids" as a basis of vegetable life, Sachs found "a real unity as a basis for the plant body," when we allow an energid to be "one nucleus with that protoplasm which surrounds it and which is commanded by the same nucleus." Then, looking forward, we shall see as one of the necessary results that the cell, often containing more than one nucleus, is really an organism, never an organ. Even without this deduction we may acknowledge the cell as an organism, because it acts as an organism.

Mechanics not being life, life is not mechanics; physiology alone is the science of the functions of life. Therefore, to understand the latter we must find a good physiological foundation for it.

By this explanation I hope to have been able to show that investigations in the life of the plant cell ought to be brought into another trace in the future. More than usual plant physiologists must be aware that they want—as Sachs says—"a scientific language, according to the true scientific idea."

LETTERS TO THE EDITOR.

* * * Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.

On request in advance, one hundred copies of the number containing his communication will be furnished free to any correspondent.

The editor will be glad to publish any queries consonant with the character of the journal.

An Alleged Mongoloid Race in Europe.

ALTHOUGH it is not usual, and often impracticable, for writers to reply directly to the various criticisms passed upon their books, yet, as an interested reader of *Science*, I may perhaps be allowed to say some words with regard to a review of my "Testimony of Tradition," contributed to your issue of Feb. 10 (p. 82), which I have not had an opportunity of seeing until to-day. This I desire to do in order to remove more than one misapprehension of my meaning in the work reviewed.

"The very slender basis for the whole theory," says the reviewer, "is the syllable *Fin*." In this he is greatly mistaken. Linguistic comparisons in this direction are certainly made, and considerable stress laid upon them, but these are entirely subsidiary to the important statements quoted in the first chapter. Briefly, these are to this effect: Wallace, a clergyman in Orkney during the second half of the seventeenth century, states that "Finn-men" were at that time occasionally seen off the coasts of Orkney, each "Finn-man" being the solitary occupant of a small skiff. In particular, he specifies the years 1682 and 1684, and another writer (Brand), who confirms his account, gives instances in or about the years 1700 and 1701. Their skin-boats, and the dress and usages of the people themselves, as described by these writers, identify them at once with Eskimoes, i.e., an Eskimo-like race. Of this there can be no reasonable doubt. Both writers state that one of their skin-boats was then preserved "as a rarity" in the Hall of the Edinburgh College of Physicians, and it is added that another specimen was preserved in the parish church of Burray, Orkney. The former statement is confirmed by an entry of the year 1696 in the minute-book of the Edinburgh College of Physicians, which I copied from the original writing and published in my book (p. 10). The writer first quoted (Wallace)

is stated—in quite another connection—to have been noted for his “veracity;” and this is quite borne out, in this instance, by the evidence of two contemporary writers and the minute-book of the College of Physicians.

That these skiffs were “kayaks,” and that their occupants were practically Eskimoes, is what I have never seen called in question. The first writer (Wallace) conjectures that they actually *were* Eskimoes from Davis Straits. This also is the explanation offered by some critics, whose preconceived notions prevent them from entertaining the idea that certain European castes, within comparatively recent times, may have been (ethnologically) Eskimoes. Wallace’s son, editing his father’s book in 1693, thinks it “a little unaccountable how these Finn-men should come on this coast,” from so great a distance as Greenland. And Brand (1701), who calls them also “Finland-men,” regards it as “strange” and “wonderful” that they should come even from Finland,—assumed by him to be their home. Orkney tradition, which styles them “Finns” and “Fin-folk,” connects them with a certain island in Orkney, with Shetland, with Norway, with the Faroe Islands, and even with Iceland.

It is perhaps within the bounds of possibility that Greenland “kayakers” made their way to the Orkneys, *via* Iceland, the Faroes, and Shetland, about the year 1680. But this assumption seems to me so unreasonable that I cannot entertain it. Were the “Finn-men” of 1701 the immigrants of 1682, and had they been living in retirement about the Orkneys all that time? Or, when pursued by the Orkney fishermen, as they often were, did they retreat on each occasion to Greenland? But it is futile to suggest questions such as these. It is much more reasonable to assume that the stories about “Finns” and “Fin-folk” (though blended in modern times with impossible stories about seals) have an actual historical basis, and relate to a people whose home was in Europe and not in America.

I dare not trespass further upon your valuable space, or I would say more with regard to the various points selected for criticism. I shall only add that the reference to the Delaware Finns of the seventeenth century is not my own, but is *quoted* (at p. 36); that “Finn” and “Lapp” were once used interchangeably, though now distinct; and that, according to C. F. Keary (“The Vikings,” p. 157), the Scandinavian peninsula, almost as far south as the 60th parallel, “was Lapp or Finnish territory” in the ninth century, which allows of possible surviving remnants at a much later date in that region,—not to speak of the British Isles.

DAVID MACRITCHIE.

Edinburgh, March 7.

A Possible Source of Confusion as to the Origin and Character of Certain Shells.

It is quite possible that in studying the fossils of a single stratum of rock or even so small a fragment as a hand specimen, one may find examples over which he pauses. Wide divergences may exist between shells that lie side by side. They have evidently been deposited from the same waters. Apparently, they have flourished under like surroundings of depth and character of water. Yet one example may bear traits of fresh-water origin, while another may be as distinctly of marine growth.

The key to the anomaly may probably be found in what is now going on along our lake shores. Take as illustration the intermingling of marine and lacustrine forms on the borders of Lake Champlain. In favorable places there are found closely packed accumulations of unios and related shells. The waves that have brought these to the shore have at the same time been gnawing at the banks of clay of the Champlain epoch. In these are imbedded saxicava and associated forms. The clay is worked over by the waters; the finer particles drift out into the lake, the coarser with the liberated shells sink down among the unios. So a firm stratum is made from forms now existing in the waters and those that long ago flourished there. These deposits await the phenomena that have consolidated like ones along shores in older geological time, after which shells of different origin and character may be broken from the same rock.

HENRY M. SEELY.

Middlebury, Vt.

BOOK-REVIEWS.

A Microscopic Study of Changes due to Functional Activity of Nerve Cells. Reprinted from the *Journal of Morphology*. By C. F. HODGE. Boston, Ginn & Co.

THE present investigation is the beginning of a new line of research, and Professor Hodge is to be congratulated on his successful pioneer work. It consists of an account of a long series of patient observations made upon the spinal nerve-cells of the frog and the cat under the influence of stimulation through the spinal nerves. The general conclusion is that stimulation of the nerve-cell produces changes, in the structure of the cell, which are visible to the microscope. The most noticeable and tangible of these changes is the shrinking of the nucleus. This shrinking of the nucleus was seen in all of the experiments described, and that it was not a pathological change was proved by the fact that a rest after the stimulation caused in a few hours a recovery of the nucleus to its normal size. Perhaps the most interesting results of the whole series of experiments was a comparison of the nerve-cells of the spinal cord and brain in animals killed in the morning after a night’s rest, and similar animals killed at night after a day’s activity. In every case a very striking difference in the microscopic appearance of the nerve cells was manifest. The whole line of work is extremely suggestive and very promising of important results in the future.

The Naturalist on the River Amazons. By HENRY WALTER BATES. With a memoir of the author by Edward Clodd. Reprint of the unabridged edition. New York, D. Appleton & Co. 395 p. Map. 8°.

AMONG the thousands of volumes that crowd the shelves of our great libraries there are few that have ever reached the honor of a second edition. Fewer still attain a third and fourth, and rare indeed is the instance of one that, decade after decade, and generation after generation, continues to delight the human soul. The vast majority of printed books are as ephemeral as the May-fly, born and dying in the same hour, read and forgotten as we read and forget the gossip of a Sunday paper. Those volumes that, no matter how often they are reprinted, are always fresh and new, and which give delight to the younger as they did to the older generation, we christen “classics.” Some have come to us from ancient Greece and Rome: others from the Middle Ages: some from more recent days. In no single century, however, are there more than a small number that ever reach the pinnacle of public approval and become designated as classics. The more books there are the greater the numbers that are cast aside; so that in our time, when thousands of volumes are being poured from the press year after year, the chances that any one will be successful in achieving the highest honor are slight indeed. A book must possess more than usual worth: give to the jaded world some new ideas, and be couched in language to be read by old and young with equal pleasure. Books like the one at present under review belong to that category which includes such volumes as White’s *Selbourne*, Darwin’s *Voyage*, and Wallace’s *Malay Archipelago*,—books which have fulfilled the requirements of classics, and which have been accorded that title by a grateful public.

No one can err, we believe, in placing Bates’s “*Naturalist on the River Amazons*” among the foremost books of travel of this age; and no one who has read it, but recalls its graphic pages with delight. Pages that bring to those who have not seen with material eyes the wonders of the tropic zone, images of delight; and that recall to those who have seen these wonders visions of never-to-be-forgotten pleasure. It is said of the ornithologist Gould, who had long desired to visit the forests of the Amazons, that, meeting Bates after the appearance of his book, he exclaimed: “Bates, I have read your book; I have seen the Amazons!” It is now thirty years since the first edition appeared. Since then many others have been printed, mainly based, however, upon the second edition. This, upon the advice of his publisher and to his lasting regret, Bates abridged to a considerable extent. The public is, therefore, most grateful to have reproduced, as in the beautiful volume before us, the unabridged words of the author,